The manuscript is extremely poorly written and needs to be largely reworked. The organization is not clear; the acronyms are carelessly defined, the arguments are hard to follow, often not supported by the data.

From what I understand, there are three main points in the paper:
      1. Fractional Fe solubility correlates with nss-SO_4^{2-}. This seems to be supported by the data, and although not a new result, it is potentially interesting for iron biogeochemical cycling. Unfortunately, terms like water-soluble and bioavailable (not the same), Fe_S and %Fe (defined only in Table 1) are used interchangeably, making it hard to assess the applicability of the results.
      2. Fractional P solubility correlates with nss-SO_4^{2-}. This part is confusing. When plotted P_S vs. nss- SO_4^{2-} biomass burning plume has much larger slope. Authors propose that this is primarily due to the difference in P sources: relatively poorly soluble apatite in mineral dust vs organic matter from pyrogenic soils. There is very little discussion regarding the organically-bound soil phosphorus, role of acidity and exchangeable Al and Fe. There are contradictory and confusing statements in the text regarding the importance of atmospheric transport and sources of phosphate for the availability of P_S. Despite higher solubility of phosphate in June samples, the text reads: "The collected Indonesian biomass burning aerosols containing Fe and P are relatively fresh, without sufficient time to react with reactive gases (e.g., SO_2 and HNO_3) in advecting smoke (Baker et al., 2006; Srinivas and Sarin, 2012), leading to lower nss-sulfate/Fe_T and nss-sulfate/P_T ratios (and in turn %Fe and %P) in the June samples."
      3. Increased Fe_S and P_S observed in the biomass burning plumes were originated primarily from pyrogenic soils. The result is potentially interesting and may very well be true. However, the only support for the argument I was able to find are the back trajectories at 500m above mean sea level (with no information on how the trajectories were derived) and the MODIS RGB picture for June 19. Notice, according to Fig. 5 airmasses collected on June 19 had minor amounts of particulates, including K^+.

Due to large number of potential oversights, I believe the regression curves for Fe_S and P_S provided in the manuscript can lead to erroneous results if used in coupled atmosphere–ocean models. I recommend rejection of the paper.

The major comments:

1. Discussion regarding Fe and P needs to be separated in the introduction as well as in the results section. Sources, atmospheric processing and removal of Fe and P need to be discussed separately. This is crucial to support the main arguments of the paper "acid processing enhances Fe and P dissolution regardless of their sources."

2. Materials and methods section is very shallow. Please include the following: A. Description of the aerosol collection; B. Description for the measurement of water-soluble fraction of Al, Fe, and P; C. Description for the ion

measurements, and D. Description for the satellite data analyses and back trajectory calculations.

3. I suggest Feb and March episodes to be separated when carrying out the statistical analyses. Fig. 5 shows that the Feb episode had high concentrations of sulfate and Fe_S and low concentrations of Fe_T, Al_T and NO_3^-, suggesting that this was a highly acidified plume. Most likely Fe_S is from the fly ash, with little contribution from mineral dust. On the other hand, the March episode looks like as a mixture of anthropogenic pollution and mineral dust. It is not clear to me though, why is the K^+ concentration similar for all three (biomass burning and non-biomass burning) plumes.

4. There is a lot of unnecessary information in the manuscript that makes it very hard to read. Please remove the descriptions of typhoon Bebinca, or endless comparisons with the other cruises in different parts of the oceans.

5. I recommend combining Figs 1 and 3a and Figs 2 (but only for AOD) and 3b.

6. Fig. 6 to 10 show three datapoints as triangles without explanation.

The specific comments

Pg. 435 ln 9. In statistics, a power law is a functional relationship between two quantities, where one quantity varies as a power of another.

Pg. 436 ln 9. The discussion regarding a portion of the atmospheric particle-bound Fe and P that dissolve upon deposition to the ocean surface seems to be irrelevant for the current paper.

Pg. 436, ln 15. Please explain what does the "soluble speciation" mean?

Pg. 438, ln 7. Please explain which satellite was used.

Pg. 441, ln. 5. Please define "%Fe"